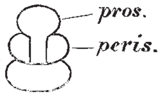
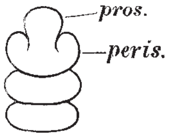
tenon arrangement which distinguishes Lumbricus from Allolobophora.





The male pore is situated normally on segment 15, but as the papillæ which carry the pores are large, they extend over the adjoining segments on either side. Earthworms vary greatly in this respect. Rosa says that spermathecæ are absent in this species, a peculiarity which has been noted in worms belonging to several other genera. I have not sufficient material to enable me to confirm or dispute this statement at present. I have counted the segments of three specimens, and found them to be in each instance 106. As the year advances I hope to be able to obtain mature adults for dissection, when it will be possible to give a detailed account of the internal anatomy. Meanwhile the external characters are amply sufficient for distinguishing the worm if the girdle is properly developed, as its nearest British ally (*Lumbricus purpureus*, Eisen) has the clitellum on segments 28 to 33. HILDERIC FRIEND.

Idle, Bradford.

The Implications of Science.

WILL you allow me to say something in answer to Mr. Dixon's letter on this subject in NATURE of January 21 (p. 272)?

(1) I admit that there is a verbal or symbolic "convention" if two (or more) persons agree to understand any given words or symbols in a way arbitrarily chosen by themselves. But the scope of such convention is exceedingly limited: if people wish to be understood, or even to understand themselves, they must use the same words as others use, and use them in the same sense (except in an infinitesimal proportion of cases). If it is said that the common application and use of current words is a mere convention, the word *convention* is taken in an extremely strained and metaphorical sense, since nothing like an explicit agreement has ever been made. The "convention" as to the use of language is as fictitious as the *social contract* of Locke and Rousseau. But in the one case, as in the other, there is a solid basis of facts, to suit which the hypothesis has been produced. Language has been moulded by thought and feeling, which, in their turn, have been impressed by facts; and it is facts and relations of facts that language seeks to express. As Mill says (in the first chapter of his "Logic") names are a clue to things, and bring before us "all the distinctions which have been recognized not by a single inquirer but by all inquirers taken together." No one, I imagine, would say that a *particular* case of the impossibility of affirming and denying a given statement, depends "solely on the law of contradiction"; but in the case of any particular assertion, the impossibility, *in that case*, is seen, and to a mind that has reached the generalizing stage, the universal is discernible in the particular. As regards the question of "real propositions," I will not occupy space with quotations, but will only refer to Mr. Dixon's letter of December 10, in which the passages occur which led me to think that he regarded assertions (or denials) of the existence of particular objects as the only "real" propositions.

(2) As regards induction, I agree with Mr. Dixon that the starting point in induction is hypothesis or discovery. But with reference to the rest of the procedure, and its relation to so-called "formal" logic, I differ from him. For I think that an inductive generalization may be set out syllogistically; *e.g.*,

> What has once produced X will always produce X;
> A has once produced X;
> ∴ A will always produce X (= all A is X).

If space allowed, I should like to consider the justification for the major premiss, and also to say something about the grounds on which the minor (which indicates the hypothesis or discovery) asserts causation [or concomitance] in a given instance.

(3) Mr. Dixon says: "We do not, in mathematics, conclude a universal proposition from a single concrete instance." But it appears to me that, as far as my own experience goes, in every concrete mathematical proposition which I understand this is exactly what happens; and I do not see how, on Mr Dixon's view, mathematical formulæ could ever have been constructed. "A mathematical formula," Mr. Dixon remarks, "does not imply the existence of any instance whatever of its application, any more than a definition implies the reality of the thing defined." But if a definition is always of a *thing*, what more is wanted? The definition is admitted to be of *something*; and what is *something* must, I suppose, exist *somehow*.

(4) I still think that in the passage in Mr. Dixon's letter which I referred to under (4) he is not consistent. For if, as he asserts, the definition of *four* as $= 1 + 1 + 1$, makes it false to say that *Twice two are four*, this is surely because the *facts* referred to by *four* are no longer what they were when the statement in question was true. If definitions were purely arbitrary, as Mr. Dixon holds, what would prevent my saying that *Four* $(1 + 1 + 1)$ *means twice two* $(1 + 1) + (1 + 1)$? It is surely only the reference to *things* which makes it absurd—(and, however *four* (4) may be defined, how is *one* (1) to be understood, except by reference to things?).

That words and symbols used intelligibly do, and must, refer to something beyond themselves, seems to me indisputable. If they did not, no assertion of the form *S is P* could ever be made, for the symbol S is certainly *not* the symbol P. And for *any* statement, of the form *S is P*, to be possible and significant, it is further necessary that S and P should have *identical* application, but *diverse* signification. If application *and* signification were the same, we should get *S is S* and *P is P*; if application were *not* the same, we must say, S is *not* P. Hence, no term can ever be taken in *mere* denotation (or application), nor in *mere* connotation (signification); but both *momenta* of each term have to be taken into account in every assertion. If (to take a case given by Mr. Dixon in his "Essay on Reasoning," p. 8) we "define" *metal* as "the list of *denotation*, iron, copper, tin, zinc, lead, gold, and silver," then *iron*, &c., can only be pointed out by taking some specimen of iron, and saying, *This and all other things which are* LIKE *it in certain respects*. An absolutely arbitrary denotation can be given only if the *whole* of the objects denoted are severally pointed out; and even then, unless they are labelled, they can only be remembered and identified by means of their *characteristics*; if labelled, by *that* characteristic.

Mr. Dixon objects to my attributing to him the view that "mathematical truths in as far as 'real' are obtained by induction, and are therefore not necessary." But in his letter of December 10 he says:—"For example, the assertion 'Two straight lines cannot inclose a space' is certainly not a 'necessary truth.' Either its terms are defined by connotation, so that its truth depends solely on those definitions, or else its terms are defined by denotation, as representing real things in space; *and the truth of the assertion can only be proved by induction from actual experience with those things*. In the first case, the truth is arbitrary, not necessary; and in the second case it might conceivably be false, as was shown by Helmholtz." It was this passage which led me to the opinion which I expressed.

Cambridge, January 31. E. E. C. JONES.

Vacuum Tubes and Electric Oscillations.

I HAVE not had the advantage of hearing the lecture of M. Nikola Tesla nor of seeing his experiments, but it does not seem out of place to recall the attention of your readers to an article by Dr. Dragoumis in your issue for April 4, 1889, in vol. xxxix. p. 548. OLIVER J. LODGE.

THE NEW STAR IN AURIGA.

SINCE our last article was written the weather has continued very bad for astronomical observations. The only new results obtained which have reached us consist of a paper read by Mr. Norman Lockyer at the Royal Society on Thursday last, and an important telegram from Prof. Pickering, which appeared in Wednesday's *Standard*.

We will take these in order. Mr. Lockyer's communication to the Royal Society was dated February 8; it stated that two more photographs, containing many more lines than the former ones, were taken on Sunday night, February 7, and it went on to make the important announcement that "The bright lines K, H, *h*, and G are

© 1892 Nature Publishing Group

accompanied by dark lines on their more refrangible sides."

This was substantially the substance of the telegram which appeared in the *Standard* on the following Wednesday (February 10), with the additional remark that the Harvard astronomers thought it possible that the phenomena presented by the new star had been caused by the collision of two celestial bodies.

On the next day the detailed observations made on Sunday night at Kensington, together with the approximate wave-lengths of the lines measured on the photographs, were sent on by Mr. Lockyer to the Royal Society. From these we learn that the Nova on Sunday appeared to be slightly brighter than on February 3.

With the 10-inch refractor and Maclean spectroscope, C was seen to be very brilliant, and there were four very conspicuous lines in the green. Several fainter lines were also seen, and a dark line was suspected in the orange. Mr. Lockyer noticed that some of the lines, especially the bright one near F, on the less refrangible side, appeared to change rapidly in relative brightness, and this was confirmed by Mr. Fowler.

Observations of the spectrum were made by Mr. Fowler with the 3-foot reflector and the Hilger 3-prism spectroscope. These call for no special remark.

Twenty bright lines have been measured on the photographs, and their wave-lengths are given in the accompanying table:—

Lines in the spectrum of Nova Aurigæ.				Bright-line stars.	Orion stars (dark lines).	Nebula in Orion (bright lines).
1st Photo. Date, February 3. λ obtained by means of a curve.	2nd Photo. Date, February 3. λ obtained by means of a curve.	3rd Photo. Date, February 7. λ obtained by means of a curve.	3rd Photo. λ obtained by means of a direct comparison with lines in α Cygni.			
K 3933	3933	3933	3933	—	3933	3933
H 3968	3968	3968	3968	3970	3968	3968
h 4101	4101	4101	4101	4101	4101	4101
4128	4130	4127	4128	—	4130	4130
4172	4172	4172	4172	—	4172	—
—	—	—	4202	4200	—	4200
4226	4227	4228	4226	—	—	4226
4268	4268	—	4264	—	4268	4268
—	—	4294	4291	—	—	—
4312	4310	4310	4310	—	—	—
G 4340	4340	4340	4340	4340	4340	4340
—	—	—	4383	—	—	4383
—	—	—	4412	—	4415	4410
—	—	—	4434	—	—	—
—	—	—	4469	4472	4472	4472
4516	4516	4522	4518	4510	—	—
4552	4552	4554	4555	4550	—	—
4587	4587	4584	4587	—	—	—
4618	4618	4625	4625	4620	—	—
—	—	4860	4860 F	4860	4860	4860

The table also shows probable coincidences with the lines in the spectra of the Wolf-Rayet stars as photographed by Prof. Pickering, dark lines in Orion stars photographed at Kensington, and bright lines in the Orion nebula photographed at Mr. Lockyer's observatory at Westgate.

In addition to the lines recorded in the table, the photographs in the spectrum of the Nova show several lines more refrangible than K. They probably include some of the ultra-violet hydrogen lines.

All the lines in the spectrum of the Nova are broad, although in a photograph of the spectrum of Arcturus, taken with the same instrumental conditions, the lines were perfectly sharp. It is also important to note that the broadening of the lines is not accompanied by any falling off of intensity at the edges, as in the case of the hydrogen lines in such a star as Sirius. With the method employed in taking the photographs, long exposures are liable to result in a thickening of all the lines on account of atmospheric tremors. The lines would also be thick if the Nova be hazy. In the photograph, however, all the lines are not equally thick.

If the lines are similarly broadened when a slit spectroscope is employed, the effect must be due to internal agitations, for if different regions of the Nova are moving with varying velocity, or with the same velocity in different directions, a normally fine line might be widened in the manner observed in the photographs.

With regard to the bright and dark lines the paper states as follows:—

"A somewhat similar phenomenon has already been recorded by Prof. Pickering in the case of β Lyræ, and this has been confirmed by a series of photographs taken at Kensington. In this case the bright lines are alternately more or less refrangible than the dark ones, with a period probably corresponding to the known period of variation in the light of the star. The maximum relative velocity indicated is stated by Prof. Pickering as approximately 300 English miles per second.

"In the case of Nova Aurigæ, the dark lines in all four photographs taken at Kensington are more refrangible than the bright ones, so that as yet there is no evidence of revolution.

"The relative velocity indicated by the displacement of the dark lines with respect to the bright ones appears to be over rather than under 500 miles per second. The reduction is not yet complete.

"Should the photographs which may be obtained in the future continue to show the dark lines displaced to the more refrangible side of the bright ones, it will be a valuable confirmation of my hypothesis as to the causes which produce a new star—namely, the collision of two meteor-swarms On this supposition the spectrum of Nova Aurigæ would suggest that a moderately dense swarm is now moving towards the earth with a great velocity and is disturbed by a sparser one which is receding. The great agitations set up in the dense swarm would produce the dark-line spectrum, while the sparser swarm would give the bright lines."

ELECTRODYNAMIC THEORIES AND THE ELECTROMAGNETIC THEORY OF LIGHT.[1]

IN a former article we endeavoured to give an account of the first part of M. Poincaré's "Électricité et Optique," in which he dealt with the electric and magnetic theories expounded in Maxwell's treatise. In Part II. he now compares the theory of electromagnetic action given by Maxwell with the somewhat more general theory put forward by Helmholtz in his celebrated paper on the equations of motion of electricity (*Pogg. Ann.*, cii. p. 529, or *Wissensch. Abhand.*, vol. i.); discusses the condition which must hold in order that the two theories may coincide; and, after a masterly exposition of the various consequences which flow from Maxwell's theory, finishes with a very valuable analysis of the theoretical and experimental work of Hertz.

In the first chapter M. Poincaré deals with the formula of Ampère for the mutual action of two current elements. The method adopted is founded on the following three principles assumed from Ampère's experiments:—

(1) That a current in a conductor may be replaced by an equal current in a sinuous conductor nowhere deviating from the first by a distance comparable with the distance of the latter from any element of the other conductor acted upon.

---

[1] "Électricité et Optique." II. Les Théories de Helmholtz et les Expériences de Hertz. Par H. Poincaré, Membre de l'Institut. (Paris: Georges Carré, 1891.)

© 1892 Nature Publishing Group